XXII. *An Account of Experiments on the Reflecting Telescope. By the Right Honourable Lord* Oxmantown, *F.R.S., &c.*

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For several years I have been engaged in a series of experiments, in the hope of increasing the power of the telescope, and I am induced, perhaps rather prematurely, to lay some account of them before the Royal Society, conceiving that, from the scale upon which they have been carried on, and perhaps from their results, they may prove interesting, particularly to those who have devoted their attention to such subjects. I should have been glad to have postponed this communication to a more distant time, so as to have rendered it in many respects more complete, but as experiments on a large scale are necessarily very tedious (months, even years, passing away almost imperceptibly), to have done so would have been to have postponed it indefinitely, and to have withheld facts which may perhaps be useful to those who are engaged in similar pursuits.

All the experiments I am about to describe relate solely to the Reflecting Telescope. With the exception of a few trifling experiments many years ago on fluid object-glasses, which led to no result, I have not had any experience in the construction of the refractor.

I have long thought that in the present state of knowledge there was not much prospect of improving the refractor to any considerable extent; the fluid object-glass, at least in my hands, did not appear promising, and the improvements which have been made in the manufacture of glass on the continent, seem to have effected little more than to afford the means of constructing larger discs of tolerably perfect glass, than was formerly practicable; still wanting, however, that exact homogeneity, and indeed those optical properties essential to any great increase of power. Upon the whole, therefore, there seemed to me to be but little chance of effecting anything really important in the present state of astronomy, except by improving the reflecting telescope; to that object, therefore, every effort was directed. The task was evidently a very difficult one, as the late Sir W. Herschel had apparently almost exhausted the subject, having devoted to it acquirements the most varied and extensive, and at the same time the most suitable, during a very long life. Still it did not seem impossible that, profiting by his labours, and imitating his example of steady perseverance, some advance might be made, trifling perhaps, but eventually leading to valuable results.

The great object seemed to be, to remove, as far as possible, the causes, owing to

which, in increasing the size, much of the perfection of smaller instruments had invariably been sacrificed. For instance, to avoid the brittleness of the best speculum metal, it had been found necessary, as the dimensions were increased, to use an increased proportion of copper, so that the alloy was inferior in brilliancy, yellower, and much more liable to tarnish*. In polishing large surfaces also there were great and peculiar difficulties, all the defects having a tendency to augment rapidly with the size, and proportionately to impair the defining power. I thought, therefore, that were it possible to discover means of securing, in the construction of large instruments, the same excellence of material and accuracy of surface, which on a small scale was attainable by the processes already published, much would be gained; but even in the construction of small instruments there was no reason to suppose that the utmost limits of perfection had already been attained. On the contrary, the unequal performance of specula wrought with the utmost skill by the same hand, and with the same materials, was a fact which clearly proved that certainty of effect, the usual characteristic of a perfect mechanical operation, was still to be sought for. It was evident, therefore, that there was ample room for experiment with a prospect of some success. With these views, the first experiments were undertaken: they were on a small scale, the earliest as far back as 1827, and some account of them appeared at the time in Sir DAVID BREWSTER's Journal for July 1828. Since that time, the scale was gradually increased, till an aperture of three feet was attained, beyond which as yet I have not proceeded.

I should, perhaps, however, remark here, that although these experiments were exceedingly numerous, they were not near so tedious as might be supposed from the length of time that has elapsed since their commencement; for in the midst of other avocations, long interruptions occurred, so that probably more might have been accomplished in one-third of the time employed continuously: there is nothing, therefore, really calculated to deter others, who have the means, from devoting their time to the improvement of the reflector: the joint labour of many may effect much, and the object is of no less importance than the advancement of those inquiries in practical astronomy, in which for further progress we must now look solely to an increase in the optical power of instruments.

In describing these experiments, it will be necessary to enter somewhat into detail, so as to enable others to repeat them without much difficulty; and as success often depends upon attention to minute particulars which have only been observed after repeated failures, it will be impossible to be as concise as it would otherwise be desirable; nothing, however, shall be inserted which has not been repeatedly tried with care, and is not calculated, either by its success or failure, to be practically useful: and first as to the best materials for constructing the reflecting surface. On this subject many experiments have been tried, but upon the whole I have little to add to what is generally known: tin and copper, the materials employed by NEWTON in the

* PEARSON's Astronomy, p. 74; and article Telescope, REES's Cyclopedia, SMEATON's Letter.

first reflecting telescope, are preferable to any other with which I am acquainted; the best proportions being four atoms of copper to one of tin (TURNER's numbers), in fact, 126·4 parts of copper to 58·9 of tin. This alloy, however, as well as every specimen of speculum metal I have ever examined, is visibly porous when carefully tested with a microscope; a very high power is not necessary: CODDINGTON's microscope, even with its lowest power, is generally sufficient; with his highest power always so. When the copper is very impure, and the alloy has been much heated, then the pores are often easily perceptible to the naked eye: contrary, however, to what might have been inferred from this, by making the copper and tin chemically pure, we do not get rid of them altogether; pores can still be detected by the microscope, even when the metal has been melted at the lowest temperature. Good cast steel is free from pores, but cannot be hardened without cracking when of considerable dimensions: all copper is, I believe, porous, tin not so.

NEWTON, with his usual acuteness, observed this porousness in speculum metal, and considered it a serious defect; and, although, where the process of polishing is conducted in the best manner, the evil which he had apprehended, viz. the rounding of the edges of the cavities, is not perceptible, still it is probable that it always takes place in some degree, however trifling, producing a defect so mixed up with other unavoidable errors as to remain undetected. The smallest defect, however, deserves attention, and perhaps others may succeed in discovering the cause of this, which I have not: my experiments lead me to think it is in some way connected with the presence of carbon or oxygen; and that possibly by watching the toughening of copper on a great scale, a process which chemistry has not satisfactorily explained, some information might be obtained.

Upon the whole, therefore, there is at present no sufficient reason for making use of copper in any other state than that in which it can be procured from the merchant, except perhaps for the small flat metals of the Newtonian; and with block-tin in the proper proportions, we have upon the whole the best material yet discovered for the specula of reflecting telescopes. It is, however, an alloy which has always been found rather difficult to manage; it is not easily cast sound, even of moderate dimensions, by the ordinary processes of the founder, or by those which have been published by scientific men; and even when so cast is very liable to break in cooling, or afterwards by exposure to any slight but sudden change of temperature, sometimes breaking even without any apparent cause.

The difficulties appeared so great, that reflecting upon SMEATON's account of Sir W. HERSCHEL's labours, I had very little hope that any very large specula could be cast and safely polished of so brittle and untractable a material; and although some of the difficulties have been surmounted, still it is probable, that in seeking to obtain the utmost possible amount of telescopic power the state of our atmosphere admits of, the requisite dimensions will not be attained by a simple casting of this alloy. The idea, therefore, obviously suggested itself of uniting several castings into one re-

flecting surface, and as the best means of effecting that, to solder them upon an alloy of zinc and copper, (in fact, upon a species of brass,) which should expand and contract in the same proportion as speculum metal.

The experiment was tried repeatedly upon a small scale, and the difficulties which presented themselves successively surmounted, till at length a speculum of three feet aperture was completed, which bears distinctly a high power.

The evil most to be apprehended in this construction, was unequal contraction and expansion in the different changes of atmospheric temperature; that, however, seems to have been sufficiently guarded against, as I have never succeeded in detecting the slightest change of figure. It is therefore highly probable, that specula very much larger may be constructed upon this plan, and that, in fact, by repeating a number of operations, each on a scale perfectly manageable, we may obtain a reflecting surface the most brilliant, and at the same time as large as can ever be usefully employed in astronomical researches. I will now proceed with the details of the construction of the three-feet speculum. I had previously ascertained that an alloy consisting of about 2·75 of copper to 1 of zinc, gave the same contractions and expansions as speculum metal; it was necessary, however, to repeat the measurement with care, using the identical materials which were to be employed in the construction of the speculum. The following simple contrivance for obtaining the relative contractions and expansions of a bar of speculum metal, and different alloys of zinc and copper, had been employed in the previous experiments, and was again resorted to.

A bar was cast of speculum metal fifteen inches long, and one inch and a quarter square; similar bars, but only three-fourths of an inch thick, were cast of the alloys to be tried, containing a little more or less zinc than the proportions I have given. A piece of brass, consisting of 2·75 of copper to 1 of zinc, was also cast and soldered to the bar of speculum metal, as represented in Plate XX. figure 1, where A B is speculum metal, C D brass, and E F the bar of alloy, with a small excavation in the lower end fitted by grinding, so as to rest steadily on the hemispherical disc G; a thin slip of brass was also soldered at the upper end of the bar of speculum metal, and the two bars made to fit neatly there, so that when brought together, a very fine line could be drawn across them with scarcely any troublesome parallax at the joint; the whole was then immersed almost to the top in a tin vessel of water of the temperature of the atmosphere, and that vessel placed in another much larger, also containing water. Pieces of ice were then dropped into the outer vessel, so that the temperature of the whole was evenly and gradually brought down nearly to 32°; and a straight line, as fine as possible, was then drawn across both bars, and examined with a microscope to ascertain that it was perfect. The temperature was then gradually raised by pouring hot water into the outer vessel until nearly 212° had been attained, and the line was again examined with a microscope, and where the alloy had been made by mixing 2·74 of copper with 1 of zinc, and the loss in melting amounted to $\frac{1}{180}$th of the whole, the continuity of the line was not broken in that range of temperature; according,

Fig. 1.

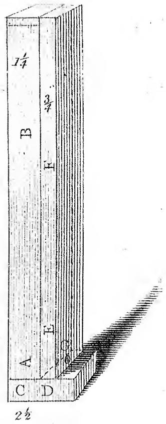


Fig. 2.

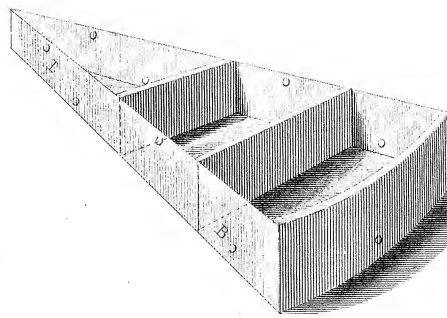


Fig. 3.

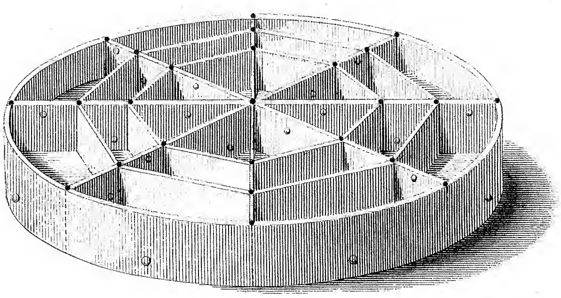


Fig. 4.

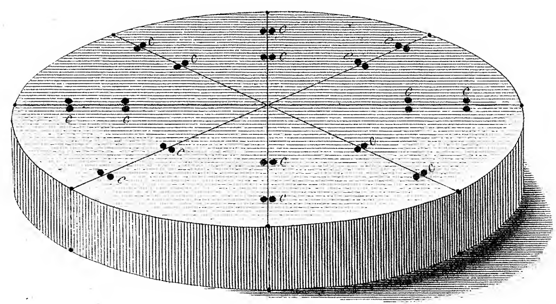
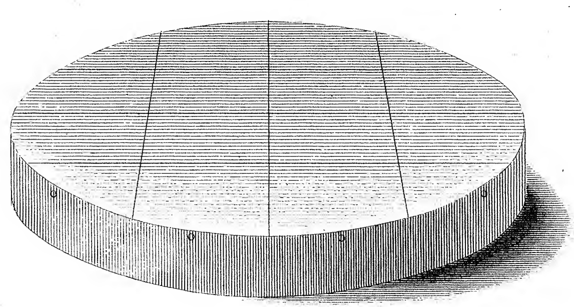


Fig. 5.



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however, as the proportion of the zinc was more or less, the expansion of the brass bar was greater or less than that of speculum metal.

As speculum metal and brass cannot be soldered together, except on a very small scale, with certainty by the ordinary methods practised in the arts, it may be useful to mention, that all that is necessary is, first, to fit the brass and speculum metal nicely, by filing or grinding, according to circumstances; the brass is then to be tinned, and suffered to cool; the surface of the speculum metal should be scraped lightly with a sharp chisel all over; the two surfaces are to be placed in contact, and matters so managed, that a slight pressure may be applied after the fusion of the tin, and continued until it has again become solid; the temperature should then be gradually raised till the tin melts; and then, but not till then, resin applied in fusion, and also a little melted tin; if resin or tin is applied in the solid state, owing to their rapid absorption of heat in becoming fluid, they will crack the speculum metal: the surfaces may be slightly separated, so as to ascertain that the speculum metal is tinned all over, which will be the case when the temperature reaches 400°: the whole must then be suffered to cool gradually.

In casting the alloy of zinc and copper, some precautions are also necessary, not practised and not required in the arts. At the commencement of these experiments, a difficulty occurred which for some time I failed in overcoming; in making the alloy of zinc and copper, and recasting it when made, much of the zinc was always volatilized, and the amount lost differed considerably each time, though the process was conducted as nearly as possible in a similar manner, so that it was impossible to allow for it, and the composition of the alloy was therefore uncertain; a fatal objection to the whole process. The furnace was the ordinary brass-founder's furnace, the air furnace of the chemist. I found, however, that the zinc was not volatilized in its metallic state; its affinity for the copper was sufficient to prevent that, but it was first oxydized. To prevent the fresh air from beating down upon the crucible whenever the lid of the furnace was removed, the furnace was made much deeper; this was of great use, but still the air which had passed through the fuel retained oxygen enough to act considerably on the zinc. Charcoal was next heaped over the crucible, still this was not effectual; at length, charcoal in fine powder was tried in a layer two inches thick on the surface of the metal, occasionally renewed; this was effectual. The crucible, therefore, must not be filled completely, and the charcoal dust can be conveniently thrown into it, whenever required, folded up in paper. If the process has been conducted with these precautions, the loss will be about $\frac{1}{180}$th, and almost exactly the same each casting.

The proportions of the zinc and copper having been determined, the brass work was first cast. By a reference to figures 2, 3, and 4, Plate XX., it will be at once seen in what way the materials were disposed of. Figure 2. represents one-eighth part of the whole seen in the reverse, a single casting; fig. 3. the whole speculum, also seen in the reverse; and fig. 4. the speculum seen on the opposite side previous to the soldering

on of the plates of speculum metal; fig. 5. the speculum complete, faced with sixteen plates of speculum metal. The whole depth of the brass work was five inches and a half, and weight about four hundred and fifty pounds; the sides I B (fig. 2.) were made square and true, and then tinned, and the whole bolted together with iron bolts, as represented in the figure; the temperature was then raised till the tin was in fusion, and the bolts tightened to the utmost, so as to make close joints; in this state the speculum was turned, plated with speculum metal, and polished; but it was found that the joints had not been sufficiently secured, owing to which the bracing had not produced its proper effect, and flexure was quite perceptible by its effects in the different positions of the telescope; on a close examination, there was reason to suspect that the solder was not everywhere perfect, though it had been so originally; that in the rough operation of bolting to the lathe and turning, it had been in some places detached, and had not subsequently united, though it had necessarily been fused when the plates of speculum metal were soldered on: it was evident, therefore, that tin alone was not to be depended upon in putting together a speculum of this size. The iron bolts, of course, contracting less than the brass, had added nothing to the security of the joints; brass bolts had been tried formerly in constructing a speculum two feet diameter, but they were not strong enough to bring the joints close, and were replaced by iron ones.

The plates of speculum metal were, therefore, taken off, and the joints secured in the following manner. The whole mass of brass work was imbedded in casting sand everywhere in contact with it; over it there were about three inches of sand. The sand was then removed from the centre, so as to expose a circular surface of about one inch and a half in diameter, and about forty-five pounds of very hot melted brass, in the same proportions as the brass speculum, were poured upon it in a continued stream from a perpendicular height of about ten inches: the sand having been so arranged, that after the melted brass had reached the depth of two inches, the remainder continued to flow off; the melted brass as it began to cool was therefore continually replaced with hot brass; and from the height at which it was poured, it was necessarily in immediate contact with the surface of the cold metal, and soon completely fused it, and perfectly united it in that place. This was repeated in thirty-four different places, marked by dots in the figures: it was also tried in the places marked *c*, *c*, (fig. 3.) but failed. When the redundant brass had been removed with a saw, and the surface made smooth, a slight hair crack was perceptible at the boundary of the fused metal, proceeding no doubt from expansion and contraction after the brass had ceased to be fluid, but before it had become ductile. This process is made use of by the brass-founder in stopping holes in defective castings; he calls it *burning*. The failure of this process at the places marked *c*, *c*, is a fact which it would be important to keep in view in disposing of the parts of a large speculum. As it was desirable that some further attempt should be made to secure the joint at *c*, *c*, holes of about one inch and a half in diameter were bored completely through at *c*, and the

intermediate brass chiseled out, as represented in the figure. They were then filled with melted brass, which was easily effected by imbedding the whole in sand; clearing the sand completely out of the spaces, and pouring the brass into each through a hole in a flat surface of sand packed in a small flask; when the brass cooled, the contraction was sufficient to draw the joint firmly together. This process may, possibly in engineering, prove useful as a means of connecting large masses of metal: I am not aware of its having before been put in practice. Conceiving that any further yielding in the joints was now impossible, the speculum was again placed on the lathe and turned to a radius of fifty-four feet, and new plates of speculum metal prepared for it.

Notwithstanding the small dimensions of these plates, none exceeding nine inches square, more difficulty was originally experienced in casting them than could have been anticipated. A great many unsuccessful attempts were made to cast them in sand, according to the directions of Mr. Edwards and others; they were seldom free from flaws, and, although cooled very slowly in an oven, they were extremely brittle, sometimes flying in pieces the moment they were touched, and generally breaking in the attempt to heat them again for soldering. The cause of this brittleness was evident, as the broken parts when replaced no longer fitted exactly, the metal therefore had been in a state of tension, owing no doubt to the edges of the plates becoming solid sooner than the centre.

The next plan tried was this: a number of equidistant thin plates of iron were immersed in a square crucible of cast iron filled with fluid speculum metal, so as to divide the whole mass into plates of equal thickness: this failed altogether, the plates of speculum metal were full of flaws; their contraction had been prevented by the unavoidable irregularities of the plates of iron, and crucible. Another plan was then tried, more successful than any of the preceding. A circular sawing machine was constructed, the blades were of soft iron, and while revolving were always partially immersed in emery and water; with this a block of speculum metal was without much difficulty cut into plates, which were perfectly free from flaws, and not liable to crack. Still their texture was not uniform; for about three quarters of an inch from their edges the arrangement of the particles was different from what it was in the remainder; so much so, that the edge of the plates for that distance evidently resisted the action of emery more than the remainder, and therefore probably a speculum made of such plates would not eventually be as true as if they were free from that defect. Upon this point, however, I cannot speak quite decisively; from what I have observed I have strong reasons to think it is so; but were it of any importance to determine it, further experiments would be necessary. I have a speculum two feet aperture formed of such plates; it was however polished long ago, and I have not a polishing tool of the construction I now make use of, of the proper size; so it has not had a fair trial. The same plates were also originally used for the three-feet speculum I am describing, but when it became necessary further to secure the joints,

they were taken off and spoiled, and in the meantime another process suggested itself, which seemed to be decidedly preferable.

It was evident that the flaws of so frequent occurrence in the plates formerly cast, and also their extreme brittleness, arose from the contraction of the metal in some places more than others, just at the time of transition from the fluid to the solid state. The edge of the plates always became solid first, and the central portions thus prevented from contracting were strained when no longer ductile. Were it possible, therefore, to satisfy the following conditions, viz. that heat should be abstracted rapidly and equally from the lower surface of a fluid disc of speculum metal, so that it should solidify from the bottom upwards in strata, or rather infinitely thin laminæ, the surface being the last to solidify, we should have a perfect casting; for the particles in that case being deposited, not uniformly, indeed, owing to the unknown action of the forces of crystallization, but in such a way as to fill up the interstices, there would be no flaws; and the temperature being uniform in a horizontal direction, and in the vertical varying in regular gradation from the lower surface to the upper, there would be no strain.

This, I believe, is the true principle upon which the most perfect castings can be obtained: its truth has been fully proved by practice; and, although in the arts fortunately it is not necessary often to attend to such minutiæ, as the materials employed are in some measure ductile, and therefore adapt themselves to the unequal strain to which they may be subjected, still it seems probable that the not unfrequent failure of large castings under a pressure much less than they were calculated to bear is due to this cause. The management of speculum metal may be regarded as an extreme case, where all the defects of manipulation are strikingly developed.

There are evidently two ways in which it might be possible to attain the required adjustment of temperature; the one by cooling the lower surface of a mould containing the liquid speculum metal, while the heat of the upper remained undiminished; the other by constructing the mould itself, so that the lower surface should absorb the heat rapidly and the upper retain it. Both were tried; the first by making the mould itself of cast iron, in which the metal was fused, and then exposing its lower surface to the action of a jet of cold water; the result justified the theory, but the mould very frequently cracked, and where this occurred before the speculum metal had become perfectly solid, the casting was spoiled; discs were, however, obtained of different sizes, the largest eighteen inches diameter, which was merely by chance, as a mould of that size almost invariably cracks before the completion of the process. The experiment, therefore, is not worth repeating, particularly as the other plan is simple and succeeds perfectly. It is obviously to make the lower surface of the mould of iron, and the upper sides of sand; at first a simple disc of iron was tried, but although the castings were sound, there was this defect; that bubbles of air were often entangled between the iron and speculum metal, producing cavities which it was tedious to grind out: the iron disc was therefore replaced by one made of pieces

of hoop-iron placed side by side, with their edges up, tightly packed in an iron frame: the edges were brought to a smooth surface of the proper curve either by the file or lathe, whichever was the most convenient. A metallic surface was thus constructed everywhere porous; as however close the hoop-iron had been packed, the interstices suffered air to pass freely through. So successful was this expedient, that of sixteen plates cast for the three-feet speculum, not one was defective: the following particulars require to be attended to. The disc of hoop-iron should be as thick as the speculum to be cast upon it, so as to cool it with sufficient rapidity; it requires to be warm, so that there may be no moisture deposited upon it from the sand; it may be heated to 212° without materially lessening its cooling power. The metal should enter the mould by the side, as is usual in iron founding, but much quicker, almost instantaneously; one second is sufficient for filling the mould of a nine-inch plate or speculum. As to the temperature of the metal, this can best be ascertained by stirring it with a wooden pole occasionally after it has become perfectly fluid; when the carbon of the pole reduces the oxide on the surface of the metal, rendering it brilliant like quicksilver, the heat is sufficient. When the metal has become solid in the ingate or hole through which it enters the mould, the plate is to be removed quickly to an oven heated a little below redness, to remain till cold, which, where the plates are nine inches diameter, should be three or four days at least.

The metal which had filled the ingates having been separated from the plates by a file, they were fitted to the brass speculum by grinding each separately by hand upon it till brought to the same curve. The surface of the brass speculum was then scraped and tinned, and when cold all resin was removed by washing it first with spirits of turpentine, and then soap and water. The lower surfaces of the plates which fitted the brass speculum, were carefully scraped all over with broad flat chisels of thin steel, perfectly hard; it is necessary that no spot should be passed over unscraped, and that the plate should be kept perfectly dry till soldered to the speculum. The plates were then arranged in their places on the brass speculum, which had been previously placed in an oven so constructed that the temperature could be gradually and equally raised. The bottom of the oven consisted of a cast-iron plate four feet diameter, set in brick work, as if it was intended for a sand bath; the brass speculum rested upon this, with loose bricks between to protect it from radiation, so that it should acquire heat solely from the heated air; the top of the oven had a cover in four pieces, that one might be opened at a time. In about eight hours the tin on the speculum was fused, and then melted resin was poured in between the plates; tin in fusion was also applied in the same manner, and the plates moved a little backwards and forwards. As soon as from the aspect of the edges of the plates it was certain that the tin was acting on the speculum metal, the fire was almost all withdrawn, and the temperature was not suffered to rise higher. The joints of the plates were now made straight, and kept open about one-twentieth of an inch by chips of wood; the whole was then suffered to cool gradually, and in five days it was ready to be ground.

There are here also some points which it is necessary to attend to. The perfect union of the plates with the brass speculum depends upon the fact, that if a plate of speculum metal, scraped as directed, is laid upon a clean surface of tinned brass, and the temperature raised a little beyond the fusing point of tin, and then melted resin and tin applied, the plate of speculum metal will be immediately tinned all over, and the union of course will be perfect. If resin, however, is applied at the beginning of the process, and therefore exposed for hours to an increasing temperature, the resin, before the temperature has reached the proper degree, will have been decomposed, and will effectually prevent the success of the operation. In my earliest experiments, I was not aware of this fact, and was therefore obliged to turn the plates over repeatedly with a wooden tongs, to remove the decomposed resin; and, although where the plates were small this was practicable, in attempting to manage the plates of the three-feet speculum I failed: several were broken by unavoidable exposure to variation of temperature. I may perhaps as well mention, that formerly I tried the muriate of ammonia instead of resin, and also a variety of other processes, but none was completely successful but the one I have given. The resin should not only be in fusion, but where the plates are large it is more prudent to regulate the temperature by a thermometer. The same observation applies to the tin; I have found, however, that a portion of unmelted tin in the ladle, in contact with the fluid metal, was a sufficient guarantee against a too great disparity of temperature. The brick work should be perfectly dry, as a drop of condensed steam falling upon a plate would certainly crack it.

Before I proceed to the grinding and polishing of the speculum, I will conclude the remaining experiments on the process of casting. The ease and certainty with which perfect plates of speculum metal of moderate dimensions had been obtained, obviously suggested a trial of the same principles on a larger scale. A perfect disc of fine speculum metal, twenty inches diameter, was immediately obtained, and recently so large a disc as three feet was cast perfect the first attempt, and has been finished without accident.

Although the principles which guided the manipulation have been fully explained, there are some practical details necessary to success, which might not perhaps suggest themselves immediately to others who have not had the same experience in managing this intractable alloy that I have had; a few remarks may therefore be useful. The disc, when cast, was about three inches and three quarters thick, and weighed about thirteen hundred weight; the metal for it was fused in two cast-iron crucibles. In my earliest experiments, in consequence of the failure of a very large crucible of cast iron, one inch and three quarters thick, made for me at one of the principal London foundries, I concluded too hastily that cast iron would not answer. The first time the crucible was tried, after it had been about four hours in the fire, the speculum metal oozed through it; when cold the defective portion was bored out and stopped with a screw; the next time the metal oozed out in several places. The crucible was

then broken, and it was found that the speculum metal had penetrated into the iron in various directions, and a small portion of speculum metal, when analysed, yielded iron. The reverberatory furnace was therefore resorted to, but I found that, owing to the rapid oxidation of the tin, the quality of the alloy was continually changing, and the result therefore was uncertain. In the mean time I found that the affinity of speculum metal for cast iron was very slight, although for wrought iron it was considerable; that the iron detected in the speculum metal was owing to the corrosion of the wrought-iron screw, with which the hole had been stopped, and that the failure of the crucible had arisen, not from the unfitness of the material, but from defective workmanship. After some experiments in my own laboratory, I found that crucibles cast in the usual way, with the mouth down, were generally defective, the speculum metal passing through minute pores not visible externally, as quicksilver through the sap vessels of wood. When the crucible, however, was cast with the mouth up, there was no such defect; we therefore have in cast iron a material capable of being formed into crucibles of unlimited dimensions. I cast one which held fifteen hundred weight, in which the speculum metal was made, but two crucibles of half the size were manageable with less machinery, and therefore were used in casting the speculum; they were raised from the furnaces by proper tackling, and placed in iron swing frames, so contrived that each crucible of metal could be thrown almost instantaneously into the mould. The fuel I make use of is wood or peat; the latter when of proper quality is preferable, as it yields a steadier fire, without the intense heat of coke, which would without great care endanger the crucibles. The mould was made in the same manner as in casting the plates, differing from it merely in shape and dimensions; it was what founders technically call an open one. The disc of hoop-iron was made circular three feet six inches diameter, three inches and a half thick, and turned upon a lathe convex to a radius of fifty-four feet. The speculum was cast with a groove round the edge, so that it might be securely embraced by a circular clamp tightened upon it with screw-bolts, to which the proper tackling could be hooked whenever it was necessary to move it.

When the metal had become solid, but was still red-hot, a strong hoop, somewhat larger than the diameter of the speculum, was placed upon it; to this hoop a chain from a windlass, passing through the annealing oven, had been previously attached, and by the action of the windlass the speculum was drawn into the hot oven, and every opening closed; in about a fortnight it was cool, and was found to be free from blemish.

It is of course impossible to ascertain *à priori*, whether it would be practicable to obtain still larger discs of fine speculum metal by this process, and polish them without accident; possibly it might, as the principal cause of fracture, unequal contraction, no longer exists. The question, therefore, will arise, whether in endeavouring to obtain still larger specula, to approach nearer that limit beyond which it is not permitted to us to pass, the better course would be to attempt, with the risk of

failure, a single casting of very great size, or at once to have recourse to the expedient of combining together a number of small castings, a process perhaps more tedious, possibly less perfect, but more certain. However, a further comparison of the two specula as to defining power, under all circumstances, and with the utmost care, will determine the future course of these experiments; at present there is no appreciable difference referable to their very different principles of construction; they both are free from flexure in the different positions of the instrument, and have defined equally well when polished with equal success. With a single lens of one quarter of an inch focus, giving a power of about thirteen hundred, they have both shown satisfactorily the dots on the dial-plate of a watch more distinctly than a very good refractor with a much lower power.

Hitherto the processes I have described were so effectual in producing the desired result, that there seems to be but little room for improvement, except in the discovery of new and better materials, an event by no means probable; but the case is far otherwise in the remaining operation, that of polishing the speculum; there, though the experiments have been even more numerous, much still remains to be accomplished.

Before the speculum is polished, it is worked to a spherical figure by a process technically called grinding, where the mutual attrition of the speculum, and a mass of nearly equal size of some hard substance, eventually produces a figure nearly spherical; and that, notwithstanding the irregularities, however great, of the surfaces of either, or both, at the commencement of the operation. Several ingenious devices have been, indeed, from time to time, suggested, more or less independent of the process of grinding, among which, perhaps, the most remarkable is that of Mr. Barton, who proposed to communicate the figure and the polish at once, by turning the speculum with a diamond, constrained, by very delicate machinery, to move in the proper path, and with a motion so slow that the resulting grooves should act on light as a polished surface; but when we recollect the extreme accuracy required, that an error of figure amounting to but a small fraction of a hair's breadth would destroy the action of a speculum, it is scarcely to be expected that any process can succeed in practice, which has not, like that of grinding, a decided tendency to correct its own defects, and to produce a result in which the errors may be said to be infinitely small in comparison with the errors in any of the previous steps from which it was derived. I need, perhaps, therefore hardly say that all my experiments have been directed to the one object, that of endeavouring to improve the common published process of grinding and polishing, particularly in its application to large surfaces; for although the accuracy usually attained is so great that we fail in detecting by mechanical means, among a variety of specula made at different times and by different persons, any deviation from the proper figure, still by optical means, in fact, by trial in a telescope, the defects are at once apparent; and we shall probably find among them examples of every grade of defining power, from the speculum which is almost perfect, to that which does not define at all. This dif-

Fig. 7.

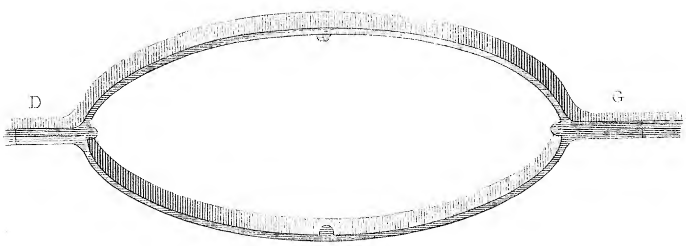


Fig. 6.

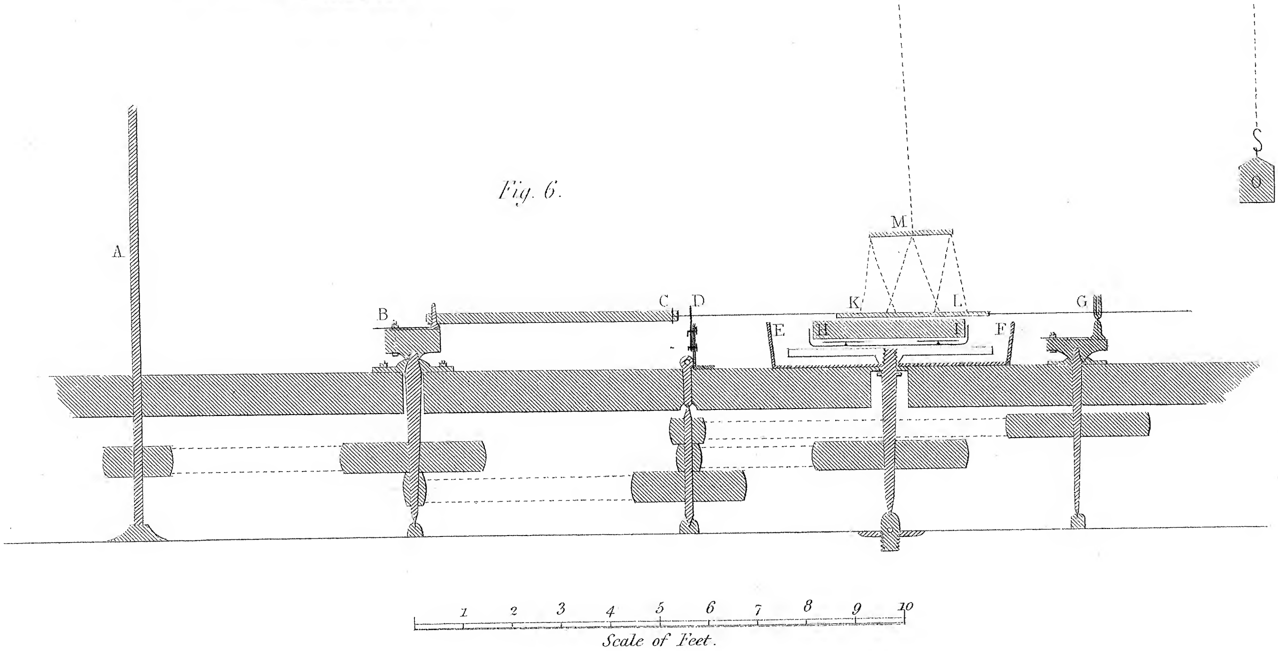


Fig. 8.

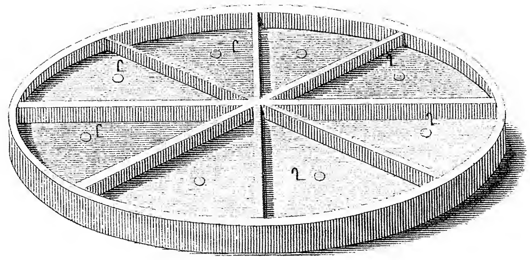
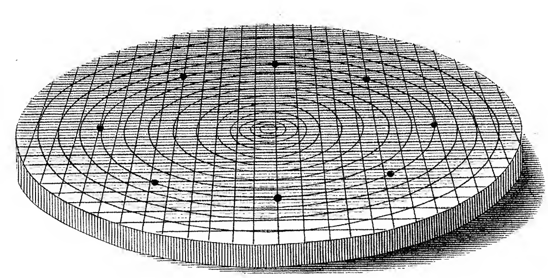


Fig. 9.



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ference, so great, is mainly owing to a variety of minute circumstances incidental to the process of polishing, which influence the result in a greater or less degree; among which, the most important are, variations in the extent or relative velocities of the motions by which the necessary friction is produced; alterations of temperature; or some accidental pressure during the process. With a view of obviating these causes of uncertainty at a very early stage of these experiments, a machine was constructed for grinding and polishing, where the different motions were susceptible of separate adjustment, and were all under complete control. The first trials with it were upon the whole satisfactory, and I sent a sketch of it to Sir D. Brewster's Journal for October, 1828, in the hope of directing the attention of practical men to the subject, and perhaps of raising a doubt in their minds as to the justness of a very deep-rooted opinion, that specula could not be polished successfully except by the hand; an opinion which, if unfounded, must necessarily have been a serious obstacle to improvement, by precluding the use of the only means available in making accurate experiments under circumstances identically the same, or, indeed, of trying any series of experiments on a large scale. The machine was soon after enlarged, so as to be capable of working a speculum three feet diameter as its maximum, and otherwise improved, and since that no further alteration has been found necessary. From an experience of several years, during which specula have been ground and polished with it many hundred times, I can safely say that it fully answers the purpose, and I believe, in working large surfaces, a degree of precision can, with certainty, be obtained by it, unattainable by the hand, even by accident. The machine, in its present state, is represented in Plate XX. fig. 6, and Plate XXI. where A is a shaft connected with a steam-engine; B an eccentric, adjustable by a screw-bolt to give any length of stroke from 0 to 18 inches; C a joint; D a guide; E F a cistern for water, in which the speculum revolves; G another eccentric, adjustable like the first to any length of stroke from 0 to 18 inches. The bar D G passes through a slit, and therefore the pin at G necessarily turns on its axis in the same time as the eccentric. H I is the speculum in its box immersed in water to within one inch of its surface, and K L the polisher, which is of cast iron, and weighs about two and a half hundred weight. M is a round disc of wood connected with the polisher by strings hooked to it in six places, each two-thirds of the radius from the centre. At M there is a swivel and hook, to which a rope is attached, connecting the whole with the lever N, so that the polisher presses upon the speculum with a force equal to the difference between its own weight and that of the counterpoise O. For a speculum three feet diameter I make the counterpoise ten pounds lighter than the polisher. The bar D G fits the polisher nicely, but without tightness, so that the polisher turns freely round, usually about once for every fifteen or twenty revolutions of the speculum, and it is prevented by four guards from accidentally touching the speculum, and from pressing upon the polisher by the two guides through which its extremities pass. In fig. 7. this bar is on a larger scale. I have tried a variety of contrivances for connecting the machinery with the polisher, but

the one I have described is by far the best. The wheel B makes, when polishing a three-feet speculum, sixteen revolutions in a minute; to polish a smaller speculum the velocity is increased by changing the pulley on the shaft A. The machine is in a room at the bottom of a high tower, and doors can be opened in the successive floors, so that a dial-plate of a watch placed perpendicularly over the speculum can be examined at any moment. The dial-plate is attached to a mast, so as to be much higher than the tower and about ninety feet from the speculum; and a small flat metal and eye-piece, with its proper adjustments, completes the arrangements for a Newtonian telescope. This simple contrivance has greatly facilitated the progress of these experiments. As appears in the plate, all the motions are produced by bands three inches wide, instead of the more permanent gearing of cog-wheels: although bands are liable to break, I think they are preferable to cog-wheels, because in a machine like this, which is for experimental purposes, if any part should become fast, which has happened more than once, the band falls off or breaks, and no mischief is done: it is only in a manufactory, where there are no experiments, but merely a routine of unvaried operations, that such accidents can be guarded against. One-horse power is quite sufficient to drive this machine while it is working a speculum of three feet diameter. The engine, however, is three-horse power, as from the distance of manufactories, it has been necessary to execute all the turning and casting work in my own laboratory.

The first serious difficulty which presented itself in polishing specula of considerable size, according to the common process, was this: when the layer of pitch was thin, as it must be to produce a good figure, however accurately at first it fitted the speculum, it soon ceased to do so, and the polishing did not of course proceed properly. This derangement, which in the ordinary mode of polishing by the hand is perceived at once by the feel, is as soon perceptible with the machine, because minute bubbles from the air, which has insinuated itself between the speculum and the polisher, are immediately observable. For some time this difficulty was exceedingly puzzling, and it was not until after many abortive attempts that the cause became evident: during the operation of polishing, the abraded matter, mixed with the polishing powder, is in part taken up by the pitch, but not equally over the whole surface; as, however, pitch is not sensibly elastic with a moderate pressure, wherever most is taken up, there the surface will be most prominent, and the figure of the polisher destroyed, unless the pitch can spread laterally. To allow of this lateral expansion a certain thickness of pitch is necessary, and I found, as might indeed have been anticipated, that the thickness required to be increased with the size of the speculum; in fact, if I may be allowed so to express myself, that the necessary thickness was some function of the diameter of the speculum. By using a layer of pitch sufficiently thick, a solid speculum of twenty inches aperture and a divided speculum of twenty-four inches aperture, the subjects of these and many other experiments, were made to define tolerably with a low power, and, at the same time, had acquired a high polish.

Mr. Edwards mentions that he had found, that, unless the pitch was of sufficient thickness, it would not preserve its figure; he had observed the fact correctly: in assigning the cause he was evidently in error, as he attributed it to the circumstance that the thin coat of pitch, as he supposed, acquired more heat from the friction than the thicker, while the reverse must have been the fact, as the thinner the imperfect conducting material, the quicker the metallic plate, to which it was attached, would have dissipated the heat, by radiation and conduction.

Mr. Edwards's specula were very small, and he found the thickness of half-a-crown sufficient. It was, however, evident, both as a deduction from all that had been written upon the subject, and from some experiments on small specula, that the necessity of using a layer of pitch thicker in proportion to the size of the speculum was a great evil, and was alone sufficient to make it impossible to polish large surfaces as accurately as small ones. A consideration of the theory, which I have ventured to put forward, suggested a very obvious remedy. It was very evident that by grooving the layer of pitch, provision might be made for its lateral expansion, wherever required, without so great a thickness. The experiment was first tried by reducing the thickness of pitch one-half, and making furrows in it by means of a hot iron quite down to, the metallic plate; the furrows were two inches apart, and there were two sets at right angles to each other, so that there were nowhere more than four square inches of pitchy surface in continuity. The result was, that the defining power of the speculum was immediately much improved. After many trials, however, a far better mode of effecting the same object suggested itself. The furrows were with difficulty kept everywhere open, and where there was a failure in this respect the old evil recurred, the polisher lost its figure; moreover, it was not found practicable to reduce the thickness of pitch to a minimum, which was a great object, and there were other minor practical inconveniences. These defects were all remedied by dividing the iron disc itself instead of the pitch; this could be done to any degree of minuteness required, and the continuous pitchy surface so reduced that its thickness might be made a minimum, in fact, not greater than necessary to satisfy the condition of enduring the small amount of abrasion which takes place during the time required to complete the polishing. The improvement which immediately followed this simple device was far greater than could have been anticipated, and the divided three-feet speculum, after this change, defined better with a power of 1200 than it had done before with a power of 300. Several polishers were made on this construction, the arrangement and dimensions of the grooves being somewhat different: that last used in polishing the two three-feet specula I think is the best, and a drawing of it has been annexed, figs. 8 and 9: the circular grooves were turned with a slide rest and are three-eighths of an inch deep and one quarter wide, leaving bands of continuous surface one quarter of an inch wide; the grooves at right angles are about one inch and a quarter apart, one quarter of an inch wide, and half an inch deep; they were cut with a small circular saw, under which the polisher was made to traverse on the bed of a large lathe.

The speculum was of course truly ground with the polisher first, and then the layer of pitch or resinous composition applied, the grooves remaining empty.

There are two conditions which I have found essential in producing a successful result; the one, that the polisher should fit the speculum exactly during the whole process; the other, that the resinous surface in contact with the speculum should be as hard as possible consistent with its admitting the polishing powder to imbed itself in it: without an attention to both of these, however accurately the motions of the machine may have been adjusted, however nearly a general parabolic figure may have been attained, the speculum will not define well. The first condition is satisfied by grooving the polisher, provided the resinous surface is sufficiently soft to expand laterally into the grooves when necessary; but when it is so, I have not found it hard enough to give a very true surface, and therefore the second condition is not satisfied. But here it is necessary to explain the meaning which I attach to the words *true surface*, in contradistinction to *accurate general figure*. A true surface is one which observes the law of continuity, when, in fact, the normal to the surface everywhere cuts the axis in conformity to the law of the curve, whatever that may be. In practice, the defect, which I call an untrue surface, is perceptible at a glance where it is very considerable, and the speculum is of long focus, for instance, twenty-seven feet; it is then only necessary to place the eye a few inches within the focus, while the speculum is turned to some bright object, as for instance the enamelled dial-plate of a watch, or the moon. The irregularity of the whole surface will then be apparent, more at the joints where the speculum is in separate pieces, but still more where there is a flaw or crack. The cause is obviously this: the metal under equal friction wears everywhere unequally, and therefore the inclination of the minute portions, I might almost say elements, of the surface, deviates slightly, but sensibly, and quite irregularly from the general curvature, producing an aberration independent either of general figure or aperture. A surface of speculum metal yields in the same irregular way to the action of acids, as indeed all metals do, but the more so as their texture is crystalline or fibrous. In proportion as the resinous surface is soft, and the polisher heavy, the irregularity increases, and therefore we should conclude that the harder the surface and lighter the polisher, the less the defect; and such is the fact. The accuracy of the *general figure* depends mainly upon the motions of the machine and the thinness of the resinous surface. If the resinous surface is so hard that the particles of polishing powder no longer sink into it deep enough to be held fast, then the polish is destroyed, the polishing process passing into that of grinding; long, however, before that limit of hardness has been attained, the resinous surface has lost its essential quality of expanding laterally, and therefore of preserving its exact coincidence of figure with the speculum. I have found that the two properties apparently inconsistent with each other, can be imparted to the polisher at the same time, simply by using the resinous composition of two different degrees of hardness, so as to form two very thin strata, the outer one being the harder. The resinous surface

in contact with the speculum can thus be made as hard as necessary, while the thin subjacent layer of softer resin expands laterally, so as to preserve the figure of the polisher. The process of polishing for optical purposes appears to me in some measure to resemble that of filing, the polishing powder imbedded in the resinous surface representing the teeth of the file: while the polisher preserves its figure exactly, and consequently its contact with the speculum is exceedingly close, every particle of polishing powder, as it insinuates itself between the rubbing surfaces, must be instantly forced into the resin, deeper as the resin is softer, producing a grooving or grain in the speculum, finer, if the fineness of the polishing powder is given, in proportion to the softness of the resin, and, consequently, to the depth the particles have become imbedded, and, therefore, the smallness of the portion of each which projects; but the moment the figure of the polisher ceases to be exact, then the polishing powder is no longer forced into the resin, but runs loose, producing a grain perhaps as coarse, or coarser than the size of the particles of the powder itself. Hence, therefore, in practice it is of no less importance to the production of a fine polish, than it is to the production of a fine figure, that the polisher should very exactly fit the speculum during the whole operation. I find invariably that the moment that exact coincidence ceases, the polish rapidly declines, and is soon completely spoiled. I have hitherto observed that the quality of the polish which yields the maximum of defining power is that which is technically called a black polish, provided a very fine grain is perceptible when the speculum is placed near a window. A speculum may be polished so that the surface appears black, and without grain, like a surface of quicksilver; but I have always found it necessary for that purpose, to employ a softer resinous composition than seems consistent with the production of a very true surface. Conceiving that such a polish, though I did not find it reflected more light, was likely to reflect more accurately, I tried a vast number of experiments with the view of obtaining it in conjunction with the truest surface, but hitherto without success: the subject, however, perhaps, deserves to be pursued further, and as it seems impracticable without injury to soften the resinous surface, the best chance seems to be to search for some polishing substance consisting of smaller particles than the fine peroxide of iron, the one I have always used, so as to produce a grain not exceeding the magnitude which theory has assigned as that of an undulation of light. In preparing the resinous surface for the polisher, I have for a long time employed a mixture of common resin and turpentine, instead of pitch, having previously experienced much inconvenience in polishing large surfaces from the gritty particles which the pitch I was in the habit of using very frequently contained. However, whether pitch or resin be made use of, it is absolutely necessary that the hardness should be adjusted to the proper standard with great care.

Mr. Mudge and Mr. Edwards have given different directions on this subject; Mr. Mudge recommending the pitch to be rather soft, and Mr. Edwards very hard; but in the common mode of conducting the operation, no precaution being used to

prevent the temperature of the speculum from progressively increasing from friction, at length a point will necessarily be attained, sooner or later, according to the hardness of the pitch, when the pitch will yield so as accurately to fit the speculum, and the polish will then rapidly improve: but should the temperature rise further before the polish is complete, the pitch will have become too soft to work a very true surface, and the speculum, even though in every other respect perfect, will not define very sharply. It is very evident that, under such circumstances, a steady temperature could not be maintained, except where the heat evolved by friction exactly balanced that dissipated by radiation and conduction, and that the result even on the limited scale of Mr. Mudge's and Mr. Edwards's operations must have been uncertain; but in working large specula, the uncertainty was so great, that it gave rise to difficulties which I found it impossible to combat, and therefore I resorted to the simple expedient of making the speculum revolve in water, kept at an uniform temperature, generally 55°: all change also in the figure of the speculum, from variation of temperature during the process, was thus at the same time prevented. The hardness of the resinous surface was therefore adjusted to suit the temperature, which is thus easily effected. Common resin is melted, and when nearly boiling, spirit of turpentine is added to it, perhaps about one-fifth of its weight; but resin varies so much in quality, that there is no guide except actual trial. When the mixture has been incorporated by stirring, a cold piece of iron is to be immersed in it, and then placed for some minutes in a vessel of water at a temperature of 55°; if then a moderate pressure of the nail makes a decided impression without splintering, it is of a proper hardness for the first layer on the polisher, and only requires to be strained through canvas. I know of no mode which in practice answers better than the very rude one of judging of the hardness by the effects of the pressure of the thumb nail: there are others more precise, but they all take too much time, and sufficient accuracy can be attained without them. For the second layer, it is mixed with one-fourth of wheat flour, which, by increasing its tenacity and diminishing its adhesiveness, prevents that accident complained of by practical men, viz. the separation of minute particles of pitch from the polisher, which afterwards run loose between the polisher and the speculum. It is to be boiled till the water of the flour has been expelled, and the mixture becomes clear, and the boiling further continued till some of the turpentine has been driven off, and the mixture has become so hard, that at a temperature of 55°, a very strong pressure of the nail makes but a slight impression: it is still too soft, and I then add to it an equal weight of resin; it will then be hard enough to produce a very true surface, and, at the same time, soft enough to suffer the particles of polishing powder to imbed themselves, and consequently to produce a very fine black polish. Whenever the resinous mixture is remelted, I suspend the vessel to the beam of a scale, counterpoise it, and take care to apply the heat so gradually as not to drive off any of the turpentine, which is immediately perceptible by the disturbance of the equilibrium. To apply the resin, the polisher is first heated to

about 150°, and the soft mixture laid on with a large flat brush, to about the thickness of one-thirtieth, or one-twenty-fifth of an inch; it is then suffered to cool to about 100°, and the hard mixture applied in the same way and to about the same thickness. When the temperature has sunk to 80°, the polisher is placed on the speculum previously covered with peroxide of iron and water, of about the consistence of thin cream. It may be well to observe, that the speculum is not endangered by applying a polisher at 80°, while it is but 55°, because the thin layer of resin retards very much the transmission of heat; but in grinding, where there is no resin, were there such a disparity, the speculum would be broken. In grinding the three-feet speculum formed in separate pieces, the iron plate happened to have been washed with warm water, and though but little warmer than the speculum, the moment it was put on several of the plates cracked, but from the construction it was but little injured: had it been the other speculum in one piece, it would of course have been destroyed.

I prepare the peroxide of iron by precipitation with water of ammonia from a pure dilute solution of sulphate of iron; the precipitate is washed, pressed in a screw press till nearly dry, and exposed to a heat which in the dark appears a dull low red. The only points of importance are, that the sulphate of iron should be pure, that the water of ammonia should be decidedly in excess, and that the heat should not exceed that I have described. The colour will be a bright crimson, inclining to yellow. I have tried both potass and soda pure instead of water of ammonia, but after washing with some degree of care, a trace of the alkali still remained, and the peroxide was of an ochrey colour till overheated, and did not polish properly. Thomson says the peroxide of iron is sometimes of an ochrey colour, probably owing to some impurity, and I have found that the slightest trace of potass or soda produces that effect. Possibly even washing with a degree of care, too troublesome for practice, would be ineffectual in removing the last remains of the alkali, as Davy found that silica prepared with an alkali always retained a trace of it, even after the most careful washing; but this is not exactly a case in point.

Having thus endeavoured to point out as concisely as possible what I have found essential in producing a *very true surface* and a fine polish, without at all wishing it to be inferred that I consider these processes quite perfect,—for, on the contrary, I believe much still remains to be accomplished,—I will next describe the means by which I have endeavoured to obtain a very good general figure.

When I had but little experience in working specula, considering the subject more theoretically than practically, I thought that a spherical figure was the only one which could be wrought with sufficient accuracy for optical purposes, and therefore that an original spherical aberration was an unavoidable evil; but that were it possible by any counteracting means to neutralize, or even diminish it, we might have telescopes of greater aperture with a given focal length. I constructed, therefore, a Newtonian telescope of six inches aperture, and two feet focus*: the speculum was in two concentric

* Sir David Brewster's Journal, July, 1828.

portions of such dimensions, that, presuming the original figure to have been accurately spherical, each should by calculation have one-half of the whole aberration: by drawing, therefore, the central portion back the proper calculated distance by a delicate screw adjustment, and bringing the images into exact coincidence, the aberration should have been reduced to one-half. The experiment was so far successful, that an instrument of eighteen inches aperture was commenced, and the castings of the speculum in three adjustible portions were completed. In the mean time, however, with additional experience, I found it necessary to adopt new views, and was soon convinced that it was not impossible to work figures which, though not rigidly accurate, were, however, better for specula than the spherical. The undertaking was, therefore, abandoned, but the original instrument still remains, and is so far curious as showing that an adjustment of such delicacy can be practically accomplished. The experiments to which I have alluded, were made with the elliptic polisher of Mr. Edwards, a contrivance in my opinion possessing more merit than has usually been ascribed to it: I found that a speculum of four inches aperture, and eighteen inches focus, after having been polished by hand as truly spherical as I could make it, was invariably improved by working it on the elliptic polisher; however, on applying the same principle to larger specula, the result was less successful; and after a great many trials with a speculum of eighteen inches aperture, I found it would not answer. On measuring the focal length of the surface at different distances from the centre of the *face*, it was certain that the radius of curvature always increased much too rapidly towards the edge; and when the principle upon which the elliptical polisher acts is considered, it is evident that such a result might have been anticipated, and that the defect, though scarcely perceptible in very small specula, would have been very important where the dimensions were considerable.

Having observed that when the extent of the motions of the polishing machine were in certain proportions to the diameter of the speculum, its focal length gradually and regularly increased, that fact suggested another mode of working an approximate parabolic figure. If we suppose a spherical surface, under the operation of grinding and polishing, gradually to change into one of longer radius, it is very evident that, during that change, at no one instant of time will it be actually spherical, and the abrasion of the metal will be more rapid at each point as it is more distant from the centre of the *face*. When, however, the focal length neither increases nor diminishes, the abrasion will become uniform over the whole surface, producing a spherical figure. According, however, as the focal length (the actual average amount of abrasion during a given time being given) increases more or less rapidly, the nature of the curve will vary, and we might conceive it possible, having it in our power completely to control the rate at which the focal length increases, so to proportion the rate of that increase as to produce a surface approximating to that of the paraboloid. Of course the chances against obtaining an exact paraboloid are infinitely great, as an infinite number of curves may pass between the parabola and its circle of curvature,

and it is vain to look for a guide in searching for the proper one in calculations founded on the principles of exact science, as the effect of friction in polishing is not conformable to any known law; still from a number of experiments it might be possible to deduce an empirical formula practically valuable: this I have endeavoured to accomplish.

The weight of the polisher was constant, being the least possible consistent with its working properly, viz. ten pounds for a speculum three feet diameter. The distance of the counterpoising lever would obviously influence the curve; that I have regarded as constant also, viz. twelve feet, as also, in all my most recent experiments, the length of the stroke of the first eccentric B, which was one-third of the diameter of the speculum; the only variable quantity was, therefore, the stroke of the second eccentric G. Under these circumstances, the most accurate determination at which I have as yet been enabled to arrive is, that when the stroke of the second eccentric G is such as to communicate a lateral motion to the polisher equal to about ·27 of the diameter of the speculum, the curve will be nearly parabolic. The curvature I measure as Mudge did, by means of diaphragms; and when the surface is true, the separate portions of it, though the general figure may be indifferent, will define sharply, and their focus can be ascertained with great precision. If the surface is not true, the curvature cannot be ascertained with any degree of accuracy; the watch-dial, the test object, is but ninety feet from the speculum, and could not be placed higher without inconvenience; it is therefore necessary in measuring the curvature of the speculum to allow for its distance. This I do simply by calculating the spherical aberration, the radiant being ninety feet distant, at two points, one $\frac{1}{9}$th of the radius, and the other $\frac{1}{3}$rd of the radius from the centre of the *face*, deducting that from the aberration for parallel rays at these points, and endeavouring so to figure the speculum that it should be over corrected that much. Any further refinement would be but waste of time in the present state of these experiments; and although we cannot hope to obtain anything but an approximation, still the limits of error will be so small that even a large fraction of that quantity may not in practice be very important. The adjustment, however, can be made with such accuracy, that the three-feet metal at present in the telescope, with its whole aperture, is thrown perceptibly out of focus by a motion of the eye-piece, amounting to less than the thirtieth of an inch; and even with a single lens of one-eighth of an inch focus, giving a power of 2592, the dots on a watch-dial are still in some degree defined.

A watch-dial is, upon the whole, as good a test for very large specula as can be desired, as there is so much light that the magnifying power can always be increased till indistinctness is perceptible; and although eventually the performance of the instrument on the heavenly bodies, the development of new details, or the discovery of new objects, which other instruments have not reached, are the proofs that an accession of instrumental power has been obtained, still as a test to have recourse to during the progress of experiments, a watch-dial, which is always at hand, and so

near that atmospheric changes do not very materially affect the result, is a far better object.

By keeping the same principles in view, a very perfect plane speculum can be worked with facility, difficult as that operation has been found in practice. The polisher I make use of for a metal about three inches by two, is three inches diameter, divided exactly like the large polisher, but with proportionate minuteness; when the metal is polished, it is tested in the usual way by viewing an object alternately by direct and reflected vision, with a very good thirty-inch achromatic, the aperture of which has been previously contracted to an inch and three quarters. If the metal is concave, it is worked with shorter strokes for about half an hour and then tried; it will be found to have become less concave, possibly convex; in the latter case it is to be worked with longer strokes; thus with the utmost facility a metal can be worked alternately concave and convex, and, with a little practice, the limit between the two can be hit with such exactness, that even with the severe test of a thirty-inch achromatic, no deviation from the plane can be perceived, and the loss of light will be the only evidence that the rays have suffered reflexion before their incidence on the object-glass. Smaller flat metals I find it better to polish on the same polisher, several together, according to their size.

The telescope is mounted very similarly to Sir JOHN HERSCHEL'S, but in consequence of its greater size and weight, I have counterpoised both the tube and the whole machine, which makes it easily manageable; so that upon the whole, though, with the experience I now have, I believe the mounting might be improved, it is sufficiently convenient. I use it as a Newtonian, as I find that, with its large aperture and short focus, the saving of light by the Herschellian construction is not at all an equivalent for the sacrifice of defining power, at least that is the result of my present experience; the indistinctness, however, from the obliquity of the speculum, does not appear to me to be so great as I should have expected, considering the size of the circle of least confusion; for this I cannot account.

To prevent flexure in specula of moderate dimensions, I find it is quite sufficient to support them in their box on three strong iron plates, each plate being one-third part of a circular area, the same size as the speculum, and a sector of it; the plates rest at their centres of gravity on points fixed at the bottom of the box of the speculum, and therefore no flexure of the box can affect the speculum. Although the same simple means would probably be effectual for specula of the largest size, in supporting specula of three feet diameter I have availed myself of the suggestion of a clever Dublin artist, Mr. GRUBB, and, at the expense of a little more complication, have substituted nine plates for the three, resting on points supported by levers, which rest on three original points; and if flexure is thus more effectually prevented, which I think it ought to be, the additional workmanship is of no importance. This lever apparatus, however, must be exceedingly substantial, quite disproportionately so, otherwise tremors would be introduced by it, attended with the worst consequences.

In selecting the materials for this communication from a vast mass of experiments, I have endeavoured to collect together such results as were most likely to be useful, and to work the whole into a shape as practical as possible, compressing it at the same time into the smallest compass: there are many minor matters of detail, which, therefore, I have unavoidably omitted; some of them possibly might be valuable to men of science, who may be disposed to take up the subject practically with a view of proceeding further than I have done. I have, however, the less regret on this account, as, although the instrument and the laboratory where it was constructed are in the centre of Ireland, the facilities of communication are such, that those who may desire further information, can easily obtain it on the spot, and form their own estimate of the performance of the instrument.

Since the commencement of last September, when the telescope was completed and in perfect working order, till Christmas, all opportunities which presented themselves for observing, not very many indeed, were taken advantage of. A considerable number of Sir John Herschel's test objects were examined, and the performance of the instrument was quite satisfactory; but as to double stars, perhaps the most striking contrast between its action and that of other instruments, was the extreme brilliancy of the minute companions of large stars; for instance, the companion of Polaris, with six hundred, was very like Polaris itself in a forty-four inch achromatic, with a two and three quarter object glass. The companions of Alpha Lyræ, and Rigel, were brilliant objects. As to the nebulæ, though it was impossible not to feel persuaded that a larger, and equally perfect instrument would have done much more, still there was enough, I think, to justify a confident expectation, that even the present instrument will add something to the very little that is known respecting these wonderful bodies. I think I might almost venture to say that the nebulæ, 27 Messier, the annular nebula in Lyra, and what is perhaps more curious, the edge of the great nebula in Andromeda, have shown very evident symptoms of resolvability; as also other nebulæ less remarkable*. The appearance is that of a resolvable nebula in a telescope not quite powerful enough to resolve it completely. No such appearance, however, was observed till the power reached six hundred, and sometimes it was more decisive with powers of eight hundred and one thousand.

It is evident, therefore, that, except for the discovery of very faint nebulæ, an object, perhaps, of but little interest, nothing would be effected by constructing a telescope of the greatest dimensions, unless it was at the same time proportionately perfect; that a mere light grasper would do nothing.

This instrument acts very powerfully on the lunar surface, and, as might be supposed, shows everywhere a variety of details not marked in the beautiful map of Beer

* In describing the appearance of these bodies, I am anxious to guard myself from being supposed to consider it certain that they are actually resolvable, in the absence of that complete re-solution which leaves no room for error; nothing but the concurring opinion of several observers could in any degree impart to an inference the character of an astronomical fact.

and Mœdler. A much higher magnifying power can also be used with decided advantage than they say has hitherto been practicable, viz. 300*: with a power of 600, and sometimes with a power of 900, many details are brought out, not visible with lower powers.

Were it practicable to construct a rectangular prism, reflecting as accurately as a flat metal, notwithstanding the thickness of the glass, there should, I think, be a considerable saving of light: I have not as yet had time to try the experiment, but I think it worth a fair trial. Such prisms, however, do not seem to have performed as well as might have been expected, perhaps owing to imperfect workmanship.

A still greater accession of light might be obtained without sacrifice of defining power, by using the Herschellian construction, were it possible to discover some means of working approximately the surface of accurate reflexion for oblique rays. I have recently tried a few experiments on a small scale on this subject, and am disposed to think the task is not hopeless; but a course of experiments on a large scale would be required to afford a decisive result. The question is not whether such a figure could be worked as accurately as a spherical one; of that I have no hope; but whether, in practice, such a degree of accuracy might not be obtained, that the mirror would define decidedly better than if it had been spherical, and as well, or nearly so, as when worked to the best parabolic figure that can be executed, and used as a Newtonian. The principle on which I have proceeded in these experiments was simply to consider the reflecting surface sought, as a portion of a paraboloid whose axis coincided with the side of the tube, the eye-piece of course placed on the axis; and in endeavouring to work that figure, I have had recourse to no other expedient than an adjustment of the motions with respect to the position of the speculum, guided by the same view of the subject which directed the attempts to work the paraboloid for the Newtonian, varied merely to suit the altered circumstances; but in the present imperfect state of these experiments it would be waste of time to enter more into particulars. I have mentioned the subject merely for the purpose of directing the attention of others to it.

Sir William Hamilton, in his paper in the Transactions of the Royal Irish Academy for 1828, on Systems of Rays, considers the surface as the envelope of an ellipsoid of revolution, having a constant axis, but a variable eccentricity, moved in such a manner, that while one focus traverses in all directions the surface which cuts the incident rays perpendicularly, the other focus remains fixed at the point through which all the reflected rays are to pass. I have not, however, discovered any practical means of availing myself of his very original mode of treating the subject.

To conclude, I think I may state as the results of all these experiments, that specula can be made to act effectively, cast of the finest speculum metal, in separate portions,

* Bis jetzt ist eine 300 malige Vergrösserung die stärkste die man mit verhältnissmässigem Erfolge auf den Mond anwenden konnte, p. 5, *note*.

retained in their positions by an alloy of zinc and copper, as easily wrought as common brass, and that they can be executed in this manner of any required size; that castings of the finest speculum metal can be executed of large dimensions, perfect, and not very liable to break; that machinery can be employed with the greatest advantage in grinding and polishing specula; that, to obtain the finest polish, it is not necessary that the speculum should become warm, but that any temperature may be fixed upon and preserved uniform during the whole process; and that large specula can be polished as accurately as small ones, and be supported so as to be secured from flexure.

To form any other than a very vague estimate of the dimensions which the reflecting telescope may yet attain, would be impossible. Without allowing for further improvements in the process of polishing, which certainly may be confidently anticipated, I think that a speculum of six feet aperture could be made to bear a magnifying power more than sufficient to render the whole pencil of light available, and that in favourable states of the atmosphere it would act efficiently, without having recourse to the expedient which NEWTON pointed out as the last resource, that of observing from the summit of a high mountain. The construction, however, of such an instrument would be a serious task, and I should be sorry to attempt it, till, after additional experience in observing, and further opportunities of comparing the two three-feet specula already finished, I felt more competent to do justice to the undertaking. In the mean time, I hope to receive from scientific men suggestions, which would be most valuable; to continue the experiments already in progress, and to arrange the details of the mechanism necessary to render so large a tube conveniently manageable. Everything, then, having been previously determined with care, subsequent alterations would not be required; tedious experiments would not now be necessary, either in constructing the speculum, or in the less interesting but necessary task of acquiring a practical knowledge of the mechanic arts; and an instrument even of the gigantic dimensions I have proposed might, I think, be commenced and completed within one year.